

LETTERS TO THE EDITOR.

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The Non-simultaneity and the generally Eastward Progression of Sudden Magnetic Storms.

UNDER the above heading, Dr. L. A. Bauer contributes to NATURE of March 2 a letter of five columns. Referring to his Table II., p. 10, he says:—"It will be noticed that Kew is not included, for the simple reason that although Dr. Chree scaled the required data some months ago, he has not yet published them nor forwarded them to me"; and later he adds:—"Dr. Chree could not have done better than immediately to have published his own data in the same open manner that Mr. Faris had done. Instead, he labours to discredit the Coast and Geodetic Survey observations, and withholds his own from public scrutiny. In half the interval of time between the first and second presentation of his paper, had Dr. Chree chosen, he could have had at his command data from Europe and Asia which, combined with his own, would have served admirably to have tested the main contentions." The real facts are as follows. In July, 1910, I consulted the Kew curves for the dates of the fifteen disturbances treated by Mr. Faris and Dr. Bauer, and, as stated in the paper read before the Physical Society (Proc., vol. xxiii., part i.) on November 11, "I was able to identify ten of the fifteen disturbances with reasonable certainty." The measurements I then took were confined to the times of commencement of these ten disturbances. Dr. Bauer's request for data, which reached me in January, included, not merely the times, but the amplitudes of all the movements. I supposed, mistakenly, as it proves, that before publishing anything Dr. Bauer would await the data from the more remote stations, which could not well reach him in less than two or three months. I thus gave precedence to official work of urgency. Also, to get the best results possible, I had an independent set of measurements made by my chief assistant, and took a fresh set myself, and considered carefully all cases in which the times obtained differed by more than one minute, the limit of accuracy I hoped to attain. In a good many cases I was doubtful which of several small movements was the one intended, so curves were drawn indicating the movements and times, and amplitudes were given for the various alternatives. The results of this very considerable labour were dispatched to Dr. Bauer on February 25, no hint having meantime been received from him that the data were urgently wanted.

The charge that I laboured to discredit the Geodetic Survey observations is equally unjustifiable. As anyone who reads my paper will readily recognise, my criticisms were directed, not against the observational data—which, I may say, struck me as quite up to the usual standard of observations—but against the use made of them. In bringing forward these criticisms, I hoped to do better service than by collecting a number of other miscellaneous data, affected by similar uncertainties, the course which Dr. Bauer thinks it was my duty to adopt.

As Dr. Bauer takes no notice of my criticisms, I should like to indicate briefly their nature, to assist the comprehension of your readers.

Mr. Faris deduced times of commencement and corresponding velocities of propagation from Horizontal Force (H), Declination (D), and Vertical Force (V) curves separately. On the average, the H times preceded the D by 0.44 and the V by 2.3 minutes. I indicated the improbability of the disturbing force commencing with a component along one only of the three fundamental directions at a station. Normally, one must expect components in all three directions; thus systematic differences in the times of occurrence in the D, H, and V curves can hardly be real. To be visible, a disturbance must attain a certain amplitude. So-called "sudden commencements" really take several minutes, as a rule, to reach their maximum, and the apparent differences in time mean, pre-

sumably, differences in the amplitudes of the three components, or difference of sensitiveness in the three magnetographs. The neglect of this point of view may lead to serious error. To see this, take a simple case. Suppose a number of similar compasses to be placed on a straight line radiating out from an electromagnet. Suppose each needle frictionless, but its displacement imperceptible until it attains 1'. Let, now, a slowly increasing current actuate the electromagnet. As the current rises, movements become visible in one compass after another, and an unscientific observer might infer that a disturbance was being propagated outwards from the magnet with a velocity which might be very small if the current increased very slowly. The time, however, when any compass is visibly affected is determined, not by the velocity of propagation of electromagnetic waves, but by the sensitiveness of the needle, its orientation, and the rate of increase of the current actuating the electromagnet. Disturbances of the type selected by Mr. Faris are usually largest in H and least in V, and it thus appeared to me highly suggestive that the times derived from the H curves were so markedly the earlier. The tendency in all cases must be for the time shown to be late, the error being greater the smaller the disturbance and the less sensitive the magnet. If, then, on any given occasion, a disturbance nowhere large were smaller in Europe than America, we should expect the time of commencement shown by the average European magnetograph to be a little behind that shown by the average American instrument.

Another criticism I made was this. Originally—so far as I could understand—Dr. Bauer and Mr. Faris assumed differences in times at any two stations to depend only on differences of longitude. This implied that the disturbance travelled either due east or due west, appearing simultaneously at all places in the same longitude. Five American stations were arranged in two groups, the mean longitudes ($79^{\circ} 9' \text{ W.}$ and $146^{\circ} 42' \text{ W.}$) of these being regarded as belonging to two central stations 68° apart. Call these two imaginary stations A and B, the former being the nearer to Greenwich. It was recognised by Mr. Faris that a disturbance travelling, say, westwards, might pass A first, reaching B after travelling 68° , or it might pass B first, reaching A after a journey of 292° . In the former case it originated in the wide zone, 267° , separating the extreme stations Honolulu (158° W.) and Porto Rico (65° W.); in the latter case it originated in the narrow zone, 40° , separating Baldwin (95° W.) from Sitka (135° W.). The peculiarity I dwelt on was that no fewer than nine out of the fifteen disturbances were treated as if originating in the narrow zone, whereas one would have expected only two or three. Dr. Bauer does not refer to this point, but the method he now follows seems different. He again takes two central stations, this time one in America, the other in Europe, but he determines the velocity, apparently, not from the difference of longitude, but from the arc 75° on the connecting great circle. Further, he tacitly assumes that all the disturbances commence in the wide zone between the westmost American and eastmost European station, as all are assumed to traverse the 75° arc and none the 285° arc. This may or may not be a better plan than that first adopted, but it is totally different. A hypothesis which makes all the fifteen storms originate between Honolulu and Katharinenburg is obviously incompatible with one that makes nine out of the fifteen originate between Baldwin and Sitka.

Dr. Bauer's second method would avoid the difficulty I pointed out of supposing that the velocity near the poles is not merely moderate, but actually small. It has, however, this obvious drawback, that it cannot give a true velocity at all unless we suppose the disturbances to actually travel along one particular great circle connecting two arbitrary points on the earth's surface.

One of my criticisms related to inconsistencies between the times shown by different stations and the conclusions reached as to the direction and velocity of propagation. This can be better illustrated by some data used more recently by Dr. Bauer himself (*Terrestrial Magnetism*, December, 1910, Table IV., p. 225), which are much more open to criticism than those from the Coast and Geodetic stations. He takes seventeen disturbances given by Mr. Ellis as recorded at eight stations between 1882-9. Dr. Bauer forms two groups of these stations, the second

containing Bombay, Batavia (107° E.), Zikawei (121° E.), and Melbourne (145° E.). Omitting hours, here are the times in minutes assigned for the commencements at the three last-mentioned stations:—

37.0, 39.7, 8.7, 48.7, 52.7, 42.7, 20.7, 44.0, 4.7, 48.7, 46.7,
18.7, 54.7, 32.7, 24.7, 45.7, 52.7,
31.3, 32.3, 0.3, 45.3, 46.0, 35.6, 21.3, 38.3, -1.2, 41.8, 41.0,
15.3, 50.3, 27.3, 20.8, 41.3, 47.4,
38.4, 33.4, 5.1, 50.0, 55.1, 45.1, 25.1, 45.1, 0.1, 55.1, 45.1,
15.1, 50.1, 35.1, 25.1, 45.1, 45.1.

The number of 7's in the first and second lines, of 3's in the third and fourth, and 1's in the fifth and sixth, tell a tale to anyone who has eyes. It is the treatment of the data, however, to which I would direct attention. Zikawei, though intermediate in longitude, differs usually in the same direction, as regards time of commencement, from Batavia and Melbourne. Its time is earlier than that of Batavia in sixteen cases, and than that of Melbourne in fourteen cases, out of seventeen; and the mean algebraic difference is no less than 5.2 minutes in the one instance and 4.6 in the other. Instead of recognising that data such as these are useless for any purposes of high accuracy, Dr. Bauer gets out means from his two central stations and obtains an average of between three and four minutes for the time of going completely round the earth, a time markedly less than the data employed give between Batavia and Zikawei, stations differing by less than 15° in longitude or 38° in latitude.

A final criticism I might mention is this: If disturbances travel round the earth in the way Dr. Bauer supposes, why do not they quite complete the circuit, or even go round several times? In some cases, at least, the amplitudes of commencing movements at stations nearly 180° apart are very similar, so that going half-way round can have had but little effect, and movements with a much reduced amplitude would still have been conspicuous. Earthquake tremors, we know, do go round more than once.

As regards the theory vaguely outlined by Dr. Bauer (pp. 10-11), I am not surprised by his claim that "the careful reader will not fail to observe that . . . [it] is considerably different from that which Dr. Chree imputed to me." But unless I had had prophetic powers, how could it have been otherwise? The earlier theory reached or was based on a definite mathematical equation (*Terrestrial Magnetism*, June, 1910, p. 123), $v = XneD/p$, where v is "the velocity of the moving ions . . . n the number of molecules in a c.c. of gas at a pressure p dynes per sq. cm., e the electric charge carried by an ion, D the coefficient of diffusion of an ion through the gas." Of X Dr. Bauer tells us:—"Regarding the variation of X , some preliminary calculations would appear to indicate that the potential gradient of 1 volt per cm. assumed may be of about the right order of magnitude for the heights concerned" (75 km. is the height finally suggested). My primary difficulties were two. Sir J. J. Thomson, the authority quoted by Dr. Bauer, applies the formula to ions moving in the direction of the field X . Dr. Bauer makes v a horizontal velocity; but "potential gradient" is a term usually applied to the earth's vertical field, and 100 volts per metre is the value usually ascribed to it, as an average (probably a low one), at ground-level. Potential gradient, in the usual sense of the term, is known from balloon observations to fall to one-tenth of its ground-level value at heights of a few thousand feet, and at 75 km. it is usually supposed to be infinitesimal. If by "potential gradient" Dr. Bauer means a horizontal field, he should say so explicitly, and explain how it is produced and how he reaches his numerical estimate. As regards his concluding remark on the question of theory, "If Dr. Chree has something better to offer I shall be glad to know it," I labour under the disadvantage of holding the view—antiquated though it may appear to some of my contemporaries—that before advancing a mathematical theory for any supposed phenomenon, it is desirable to make reasonably sure that the phenomenon actually exists.

If we take Dr. Bauer's latest conclusions respecting the

fifteen storms he deals with in NATURE, we have nine going east and six west. The mean algebraic difference between the European and American times of commencement, + denoting earlier occurrence in America, is only +0.11 minute. In view of the fact mentioned by Dr. Bauer that the algebraic mean difference between the times deduced by two skilled observers, Drs. Venske and Krogness, for the commencements of six disturbances at Potsdam—one of the best equipped of stations—was no less than 0.4 minute, I think most physicists of experience will recognise the expediency of awaiting something much more decisive before passing a final judgment.

March 4.

C. CHREE.

TOGETHER with Mr. O. Krogness, I am just on the way to Khartoum, in Sudan, in order to carry out some researches on the zodiacal light.

In connection with these investigations, we propose to secure some magnetic records with very sensitive apparatus. Special care will be taken to obtain as accurate a time-determination as possible.

For one set of observations we shall use an hour-length of 20 mm.; for another set, with twelve times greater hour-length, records will be obtained between about 5h. p.m. and 3h. a.m. Greenwich mean time.

If at other observatories, especially near the equator, similar rapid records could be secured in the same time-interval, these would, I think, be of value in deciding the question of the simultaneity or non-simultaneity of abruptly-beginning storms.

Our records are intended to commence on March 20, and will continue for one month.

Similar records will also be obtained by Prof. S. Saeland in Trondhjem.

KR. BIRKELAND.

Berlin, March 3.

The Centenary of Bunsen's Birth.

ON March 31 it will be one hundred years since Robert William Bunsen was born, and it has been felt that that occasion should not be allowed to pass without his pupils and admirers in this country giving expression to their veneration of the memory of one of the greatest chemists of our times. A committee has been formed to make the necessary arrangements, and it was intended to celebrate the centenary by a dinner. Unfortunately, the two most distinguished pupils of Bunsen, Sir Henry Roscoe and Sir Edward Thorpe, are at present prevented by indisposition from attending such a celebration. In these circumstances the committee has decided not to proceed with the arrangements for a dinner; but, feeling that the occasion should not be allowed to pass unnoticed, they have resolved to send a signed address to the Heidelberg University from old Heidelberg students in this country, and to place a wreath on the Bunsen monument. May we request those old Heidelberg students who wish to participate in this movement to send their signatures to Prof. H. B. Dixon, of the Manchester University, the chairman of the committee, or to either of the undersigned honorary secretaries?

FRANCIS JONES (Manchester Grammar School).

J. GROSSMANN (Plymouth Grove, Manchester).

Chemical Laboratory, 157 Plymouth Grove,
Manchester, March 11.

Life and Habit.

YOUR correspondent "W. H. M." (NATURE, March 2, p. 12), who believes that it is necessary for newly hatched chicks to learn to eat by imitation, should see a litter of pigs being born. Each little pig the moment that he is outside hurries over the sow's hind legs, and, in the second second of his outdoor life, has a teat in his mouth. If the navel-cord has not got clear of his late home, he tugs away at it with all his might. Seeing such a sight, one might suppose that before birth the creature had been eagerly looking forward to his first breakfast. Or did the splendid prospect flash into his mind only as he found his feet?

FRANCIS RAM.

54 St. John's Road, N., March 13.